ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *Dec.* 10, 1852. No. 2.

Capt. W. H. SMYTH, V.P., in the Chair.

John Stanistreet, Esq., Liverpool, was balloted for, and duly elected a Fellow of the Society.

List of Variable Stars detected by Mr. Hind, with their Approximate Places for the year 1850.

No.	Mean R.A. 1850.	Mean N.P.D. 1850.	Magnitudes, Max. and Min.	Date of Discovery.
	h m s	° ′		
1	1 9 44	81 51.4	9 — 13	1851 Aug. 24
2	1 22 54	87 53.6	7.8 — 9.10	1850 Nov. 4
3	4 20 5	80 10.5	9 — 0	1850 — 14
4	4 50 50	82 6.3	9 — 12.13	1848 Aug. 24
5	6 58 20	67 4.1	7.8 — 11	1848 Oct. 11
6	7 34 2	66 12.2	9 — 0	1848 Feb. 25
7	7 40 18	65 53.8	9 — 0	1848 Mar. 4
8	8 35 22	70 24.9	8 — 10.11	1848 Feb. 1
9	8 45 44	86 22.0	8.9 — 13.14	1848 — 1
10	8 48 5	69 34.9	9.10 — 12	1850 Nov. 25
11	8 48 22	98 34.3	8.9 — 10.11	1851 Jan. 8
12	13 25 1	96 25.2	8 — 11	1852 Apr. 19
13	16 51 6	102 39.5	4.5 — 11.12	1848 — 27
14	20 2 51	104 42.4	9.10 — 0	1848 May 24
15	22 59 7	80 16.5	8.9 — 0	1848 — 30

Where dots are placed thus .... after the assigned magnitude at minimum, there is reason to suppose that the star may become fainter still. Nos. 6 and 7 have periods of about 296 and 292 days respectively (see *Ast. Nach.* No. 804). No. 8 varies in 9.02 days, or thereabouts (*Ast. Nach.* No. 804). No. 13 is the new star in *Ophiuchus* discovered by Mr. Hind in April 1848. (*Ast. Nach. No.* 832).

LUTETIA.

Elements. By Mr. W. Ellis, of the Durham Observatory, deduced from the Durham Observations of Dec. 27, 1852, and Jan. 2, and Jan. 11, 1853.

Epoch 1853, Jan. 1, 0^h Greenwich M.T.

	°	′	″	
Mean Anomaly	45	36	19·8	Mean Equinox of Epoch.
Long. Perihelion	3	41	41·9	
— Ascending Node	80	21	36·3	
Inclination	3	5	6·0	
Angle of Eccentricity	6	36	44·5	
Log *a*	0·4170508			

The middle observation is nearly satisfied by these elements.

An ephemeris by Mr. Ellis, founded on these elements, was sent at the same time; it extends from January 23 to March 6. Copies have been sent to those observers who attend to the small planets. (*See also Ast. Nach. No.* 847, p. 116.)

Letter from Mr. Lassell.

"*Valetta, 29th Dec.* 1852.

"Though I am almost afraid of *Saturn* having become an exhausted subject, yet, as he has since my last exhibited some change of phenomena, I will trouble you with another sketch of him, as he now appears, and a few more last words. Last night, after an interval of rather broken weather, the sky was unusually tranquil, and in the intervals of passing clouds I obtained views of the planets quite equal, if not superior, to any I have ever before seen. The power principally used was 650, speculum C in the tube, which afforded an image in respect of size, sharpness, and brilliancy, hitherto to my eye unexcelled.

"I have spoken highly of the definition of the other speculum, A; and certainly, under the best circumstances, I doubt if the *vision* would suffice to inform me which of the two specula was in the tube, so nearly are they alike; yet, in the average of many nights' observation, I think I incline to prefer C, from an impression of its greater uniformity of action. I doubt if I ever before produced consecutively two two-foot surfaces, so undistinguishable.

"For some time, as well as especially last night, I have noticed a peculiarity of form in the shadow of the ball of *Saturn* upon the ring, which I have endeavoured as faithfully as I can to depict in the accompanying drawing. Its curvature suddenly changes at the division of the rings, the part projected upon the ring B being almost, if not exactly, a right line. Would a *very small* amount of curvature (convexity) of the surface of ring B suffice to change the otherwise convex edge of the shadow into a right line? There cannot be a *large* amount of convexity of the surface, as it would then be optically manifest. A very similar appearance I noticed,

less distinctly, of the shadow of the ball on the western arm before opposition, mentioned in a former communication — probably it may be now more manifest, from the greater breadth of the shadow, or I have been led to notice it more. The 'spot' which I also spoke of being visible on the limb opposite to the shadow, has now no existence, and I think it may have been the first and last appearance of this angle of the shadow on the respective limbs.

"The shadow of the ring upon the ball has now become very obvious near the north pole, but exhibits a striking contrast to the shadow just spoken of in depth of shade; for while the latter is as perfectly black as anything can be, the former is only a dark grey, about the shade of the dark ring near the ansæ. In breadth, this shadow is now equal to rather more than half the breadth of ring A at that part which is contiguous to the shadow; and it appears to deepen in shade from its extreme northern edge to its junction with the ring. Probably the very oblique direction in which the shadow must fall on the rotundity of the planet may account for this paleness.

"I compared under these advantageous circumstances the two ends of the obscure ring, and indeed examined its general form, and could detect no want of symmetry or regularity of curve. In estimating, however, its similarity of form at the two ends, the judgment is somewhat disturbed by the projection of the shadow of the ball on the dark ring on the south following side. I noticed a difference of colour, the preceding end being a bluish-grey, while the following had a touch of yellow or brown in its greyness.

"The belts have altered considerably since my last letter. The division of the ruddy belt is much less plain. The first of the blue belts, which was very dark and decided, has nearly disappeared; the second, though fainter, is still traceable, while the small circular polar one remains as dark and visible as ever. I do not observe any recurring spot on the surface of the planet by which his rotation on his axis might be observed, though I have sometimes noticed that the shading on the ring B has not been the same at both ends. This, however, has not been sufficiently constant or distinct to serve any practical purpose.

"During the fine atmosphere of last night, I could not but be greatly struck with the beauty of the Saturnian picture afforded me. With the observing power of 650 nothing seems wanting to the precision of the image but the removal of the *minute flickerings* of the atmosphere, which with this power, and the aperture of my telescope, I do not recollect ever being unconscious of. The edge of the ring seems as hard and sharp as a metallic rim, and on a perfectly black ground in the quietest atmospherical moments. A sufficient proof of this, scarcely, however, to be duly estimated by those who have not been accustomed to look out for and observe the closest of *Saturn's* satellites, is, that *Mimas* was kept steadily in view, and watched up to his greatest western elongation, notwithstanding the splendour of the planet and widely-opened ring shining full in the field. I have introduced into the drawing

the approximate positions of the two nearest satellites as they appeared about $10^h\ 0^m$ Greenwich mean time. The greatest elongation of *Mimas* was estimated to occur at $10^h\ 4^m$, its distance from the end of the ring I estimated to be five-sixths of the sum of the breadths of rings A and B. This may serve as a guide to the possessor of any telescope powerful enough to give hopes of seeing this most minute and delicate object. *Enceladus* passed his inferior conjunction at about $8^h\ 12^m$ Greenwich mean time.

"This is really a splendid climate. There is now (at Christmas) just that amount of cool freshness in the air which makes exercise agreeable, and the valleys putting on their mantle of green give even the rock of Malta a fertile aspect. The enjoyment a walk affords in this transparent atmosphere, under this glorious sun, giving its warm colours to the architecture, and all manner of tints to the crystal waves as they break upon the shore, is more than my eloquence suffices adequately to convey to you.

"We all feel we have rather too much to do. People here think I am a gentleman of fortune, who has brought a large telescope here 'for his amusement!' How different from the fact: I call it real labour, and shall rejoice when my emancipation comes."

Extract of a Letter from Mr. Lassell.

"*Valetta, 10th Jan.* 1853.

"I have now to hand you the results of my observations upon *Neptune* and his satellite, in which you will see what I think of the probability of finding other satellites. With *Uranus* I have not quite done yet, though I am pretty well settled in the conclusion that he has no more satellites visible with my means than the four, so that my discoveries in this way are likely to be more negative than positive. However, I think it is high time that *Uranus's* establishment should be reduced. He has been luxuriating these many years with a retinue that I really believe does not belong to him, and, therefore, he must be cut down to four attendants, until some astronomer arises rich enough to present him with some more.

"We have the weather rather more broken now, but yet, I think, more starlight than occurs in the best seasons in England — quite as much as I have health and strength enough to use. We are astonished and delighted with the wonderful display of clusters and nebulæ a little below the equator, in the neighbourhood of *Orion*. One might be entertained and gratified by the hour, just to take the telescope in hand, and sweep slowly over the milky way east of that constellation. The full light of this telescope, charged with a power of from 160 to 200, and a field of about 20′ in diameter, with its precision of image and absence (with these powers) of sensible atmospheric disturbance, affords such pictures of the sidereal heavens as I cannot at all adequately describe, and as call forth continually from the observer involuntary exclamations of delight and astonishment."

Observations of Neptune and his Satellite.

1852.	G.M.T. h m	Position. ° ′	G.M.T. h m	Dist. ″	Reference Letter.
Oct. 28	7 54	37 6	8 8	16·62	(*a*)
29	6 43	48 36	6 56	13·19	(*b*)
30	7 17	159 est^{d}.			(*c*)
31	6 55	215 57	7 9	16·23	(*d*)
Nov. 1	6 20	223 32	6 39	12·77	(*e*)
2	7 7	9 13	7 23	4·45	(*f*)
3	6 8	37 15	6 29	15·78	(*g*)
4	6 21	49 29	6 34	11·40	(*h*)
5	7 0	177 0	7 0	4	(*i*)
6	6 16	215 42	6 32	16·57	(*k*)
9	6 26	38 34	6 40	17·20	(*l*)
10	6 41	53 52	6 51	9·60	(*m*)
11	6 15	197 53	6 41	5·26	(*n*)
12	6 2	216 36	6 12	16·81	(*o*)
15	6 4	40 57	6 21	17·42	(*p*)
16	5 58	57 17	6 15	7·08	(*q*)
17	6 13	205 12	6 25	7·64	(*r*)
18	6 2	217 59	6 16	16·99	(*s*)
19	6 4	232 51	6 15	6·69	(*t*)
20	5 47	26 28	6 2	10·93	(*u*)
26	5 37	31 46	5 50	11·34	(*v*)
29	6 6	207 18	6 16	11·36	(*w*)
Dec. 8	5 23	33 57	5 38	13·09	(*x*)
9	5 43	45 52	5 52	13·72	(*y*)
27	5 0	51 34	5 7	10·82	(*z*)

"(*a*). Sky hazy. No illumination of the field used but that given by the moonlit sky—itself rather too great. Yet the satellite was perfectly easy, and the measures taken with great comfort and confidence. The planet was well defined and round.

"(*b*). Planet round and well defined. Satellite bright. The rising moon rendered artificial illumination unnecessary.

"(*c*). On turning the telescope on the planet, he appeared as usual, attended by his satellite near its greatest elongation; and if I had not *expected* to find the satellite near its closest appulse to the planet, I might have proceeded to measure its position and distance. I did, indeed, apply the micrometer, and found the distance about 2″ more than the satellite's greatest elongation. I mention the fact, as one of the *very rare* instances in which a star of the identical magnitude is found so near the place of a satellite as to be temporarily mistaken for it; but even here, applying the micrometer at once verified the illusion.

"I proceeded to look more carefully for the real satellite, with the usual measuring power of 760, but was for a long time disappointed, and feared I must give up the search. At length I took off the micrometer and applied Dollond's polycratic wheel of single convex lenses, powers 222, 479, 628, 805, 1292, and 1917, changing the powers successively from the second or third, but failed to get a steady glimpse of the satellite until I got to the highest, 1917, with which I saw it pretty steadily and well, though the planet itself with this power was like a ball of wool. I estimated the distance of the satellite about three-fourths of the

planet's diameter from its limb, and I think it did not exceed eight or nine-tenths at the utmost. In position it appeared at right angles with the small star, whose position I measured in order to get at the angle of position of the satellite, which could not be measured directly.

"(*d*). Satellite rather faint, from the haziness of the sky. Yet, independently of this, I have an impression that the satellite is fainter than in the opposite point of its orbit, though the difference does not amount to a certainty. Power 760. Seven measures each of position and distance taken.

"(*e*). I have again the impression that the satellite is not so bright as in the opposite part of its orbit.

"(*f*). The satellite extremely close, but the fineness of the night enabled me to get six measures, well and satisfactorily, with power 760. Planet round and well defined. Applied single lens, 1292, with which the satellite was much brighter: estimated distance $1\frac{1}{2}$ diameter off. Definition very good for the power. In a fine black field I looked carefully round for other satellites, to the distance of about a minute, without finding any. [With a heavy dew, the night was delightfully calm and mild.] I am satisfied that on this occasion I should have seen a point of light considerably less than half the magnitude of the satellite, had there been one.

"(*h*). Air remarkably fine. I receive a decided impression of ellipticity in the direction of the greatest elongation of the satellite: powers 650 and 805. There is also an impression of an extremely flattened ring in the direction of the transverse axis. I think I have never seen *Neptune* so well before. [I copy this record of the appearance of *Neptune* from my observation-book, yet I suspect some illusion.]

"(*i*). The satellite was seen with 760, but much better with 1292. It was too faint and close to *measure*, in either position or distance, and the place given is the result of estimation. Atmosphere less favourable than usual.

"(*k*). Surveyed the neighbourhood of the planet round for satellites, power 1018; none appeared. The field fine and black. Certainly no point of light could be detected within three times the greatest elongation of the known satellite. A minute star, about half its magnitude, was in the field, but was proved to be a star on the 9th of November. Field of the eye-piece 3′ 36″.

"(*l*). The neighbourhood of the planet again examined, round to the distance of 3′, with power 1018, without finding any points of light which might be satellites. From the brightness of the known satellite I am more and more confirmed in the opinion that, although he probably has other satellites, none of them bears a greater proportion of magnitude to the known one than Tethys or Dione, among *Saturn's* satellites, does to *Titan:* more optical power, therefore, than I possess will be necessary for their discovery.

"(*m*). The distance, a mean of eight accordant measures; but the sky is more disturbed than usual, and clouds are passing. Power 760.

"(*n*). Measures of the satellite, from its proximity, taken with difficulty. Five measures of the diameter of the planet gave 2″·324.

"(*q*). Notwithstanding the closeness of the satellite, some haziness of the sky, and the bright moonlight, the distance is the result of nine coincident measures—the position, of seven measures.

"(*r*). The haze of the sky, neighbourhood of the moon (and *such* a moon as is not seen in England), and diffused vision, arising from an aggravated sirocco wind, rendered observation difficult.

"Dec. 7. After some interruption of the fine weather by gales of wind and occasional showers, I turned the telescope upon *Neptune*: and measured a companion, which I took to be the satellite, in position and distance—the former 328°, the latter 12″$\frac{1}{4}$. But on reducing the measures I found that I must have measured a *star*, as the true satellite at this distance never could be in this position, though the magnitude and distance might well have belonged to it. Not having any notion at the time of observing where the satellite would be situated, and the fact being that it was almost at its closest approach to the planet, and, moreover, being deceived by the star, I overlooked the true satellite. But even here the outstanding of the position angle immediately detected the error. Seven measures of the diameter of the planet with 1027 gave 2″·991.

"Dec. 8. Measured diameter of *Neptune* mean of five measures with power

1027 gave 2″·645. On going up to the observatory this evening, I observed *Mercury* very near the horizon, shining very brightly for his position. I watched him go behind the horizon as behind a transit-wire — very obvious one movement, gone the next. Such was the clearness of the atmosphere. *Canopus*, too, which has here only a meridian altitude of 1° 49′, including refraction, occasionally shines out with a brilliancy almost rivalling *Rigel*, although the latter is so far above him.

"Dec. 9. Remeasured the diameter of *Neptune* with 1027: mean =2″·774. The power, however, seemed rather too great for the circumstances.

"Dec. 20. Remeasured the diameter of *Neptune*, power 1027. The mean of six measures gave 2″·796.

"Dec. 22. Remeasured the diameter of *Neptune*. Mean of eight measures with 1027 gave 2″·450; mean of six measures with 219 gave 2″·710.

"Dec. 27. Mean of ten measures of diameter with 760 gave 2″·628. No stars or points of light visible within 4′ of the planet. Power 778. The measures of distance were obtained in very strong twilight.

"January 1, 1853. Remeasured the diameter of *Neptune*. Disk capital, and circumstances very favourable: mean of eight measures with 366 gave 3″·035; mean of five measures, power 614, gave 2″·774.

"The mean of all the measures of the planet's diameter is 2″·713. The difference of the various sets is greater than I should have expected; but there are many things which must influence the result, *e. g.* the state of atmosphere, the magnifying power, and the degree of illumination. As varieties of these have accompanied the measures, I hope this mean will not be far from the truth. The feeble light of the disk and its minuteness render its accurate measurement a task of great difficulty. The observations of the satellite, compared with former ones made at various periods, will, I believe, require a somewhat longer period than has been given, viz. about 5·8779 days.

"*Valetta, Jan.* 5, 1853."

On the Appearance of the Planet Saturn and his Rings, as viewed in Mr. Bishop's Refractor, 1853, January 9th. By Mr. Hind.

"The night of January 9th being one of unusual steadiness of definition with high powers, the planet *Saturn* was attentively examined, certainly under far more favourable conditions than at any previous date during the present winter.

"The dark ring was prominently visible under magnifiers of 108 and upwards to 460: it appeared to fill two-fifths of the space between the inner edge of the interior bright ring and the ball, being brightest near the former. Its colour was a light purple; of this point there could be no doubt. Nothing like a division in the obscure ring could be traced. During the most favourable glimpses there appeared to be a division on the exterior bright ring, rather outside the middle of its breadth, which might be traced, for some distance especially, on the eastern side of the ring. Such a division was distinctly seen at Mr. Bishop's observatory in the autumn of 1845.

"But the most evident of the newly-announced appearances connected with this planet was the visibility of the contour of the ball *through* the obscure ring where it crossed the disk of *Saturn*, as first remarked by Mr. Lassell and Captain Jacob. This phenomenon had been previously suspected on several occasions, yet

always under less advantageous circumstances than on the night of January 9. On former evenings, the contour only of the globe appeared, by momentary glimpses, to penetrate through the obscure ring; whereas, on January 9th, the appearance was such as to convey the idea that it was not merely the outline, but the whole surface traversed by the dark ring, which was visible through it.

"From observations last autumn, I had considered it very probable that the continuation of the outline of the ball through the obscure ring was an optical illusion, arising from the impression made upon the eye by the brightness of the ball in comparison with that of the ring; and though I conceive this explanation should not be hastily set aside, the results of the favourable examination on January 9th have rather shaken my confidence in it: the dark ring on this night certainly appeared to possess a *partial transparency*, which would account for the contour of the globe being perceptible up to the interior bright ring. It was impossible not to remark the much lighter (greyish) shade presented by the projection of the obscure ring upon the ball, as compared with the black shadow of the latter upon the further side of the bright rings.

"*South Villa Observatory, Jan.* 13, 1853."

Some Remarks on the probable present Condition of the Planets Jupiter and Saturn, in reference to Temperature, &c. By Mr. Nasmyth.

"The remarkable appearances which characterise the aspect of the planets *Jupiter* and *Saturn*, as revealed by the aid of very powerful and excellent telescopes, have induced some reflections on the subject of their probable present condition as to temperature, that with a view to excite more special and careful observation of the phenomenon in question, and promote some discussion on this interesting subject, I have been tempted to hazard the following remarks, which may perhaps prove acceptable to some of the members of the Royal Astronomical Society.

"In a former communication, in reference to the structure and condition of the lunar surface, I made some remarks on the principle which, as it appears to me, gives the law to the comparative rate of cooling of the masses of the planets, namely, that while the heat-retaining quality was due to the mass of the planet, the heat-dispensing quality was due or governed by its surface; and as the former increases as the cube of the diameter of the planet, while the latter increases only as the square of its diameter, we thus find that the length of time which would be required by such enormous planets as *Jupiter* and *Saturn* to cool down from their original molten and incandescent condition to such a temperature as would be fitted to permit their oceanic matter to permanently descend and rest upon their surface, would be vastly greater than in the case of such a planet as the earth.

" Adopting the results which geological research has so clearly established as respects the original molten condition of the earth, as our guide to a knowledge of the condition of all the other planets, it appears to me that we may in this way be led to some very remarkable and interesting conclusions in reference to the probable present condition of such enormous planets as *Jupiter* and *Saturn*, as may tend to explain certain phenomena in respect to their aspect.

" Assuming as established the original molten condition of the earth, and going very far back into the remote and formative periods of the earth's geological history, we may find glimpses of the cause of those tremendous deluges, of which geological phenomena afford such striking evidence, and by whose peculiar dissolving and disintegrating action on the igneous formations which at that early period of the earth's history must have formed the only material of its crust, we may in that respect obtain some insight into the source whence the material which formed the first sedimentary strata was derived. If we only carry our minds back to that early period of the earth's geological history, where the temperature of its surface was so high as that no water in its *fluid* form could rest upon it, and follow its condition from such non-oceanic state to that period at which, by reason of the comparatively cooled-down condition of its surface, it began to be visited by partial and transient descents of the ocean, which had till then existed only in the form of a vast vapour envelope to the earth, we shall find in such considerations, not only the most sublime subject of reflection in reference to the primitive condition of the earth, but also, as it appears to me, a very legitimate basis on which to rest our speculations in regard to the probable present condition of *Jupiter* and *Saturn*,—both of which great planets, I strongly incline to consider, for the reasons before stated, are yet in so hot a condition, as not only not to permit of the permanent descent of their oceanic matter, but to cause such to exist suspended as a vast vapour envelope, subject to incessant disturbances by reason of the abortive attempts which such vapour envelope may make in temporary and partial descents upon the hissing-hot surface of the planet.

" Recurring again to this early period of the earth's geological history, when it was surrounded with a vast vapour envelope, consisting of all the water which now forms the ocean, the exterior portion of this vapour envelope must, by reason of the radiation of its heat into space, have been continually descending in the form of deluges of hot water upon the red-hot surface of the earth. Such an action as this must have produced atmospheric commotions of the most fearful character; and towards the latter days of this state of things, when considerable portions of what was afterwards to form our ocean came down in torrents of water upon the then thin solid crust of the earth, the sudden contraction which such transient visits of the ocean must have produced on the crust of the earth would be followed by tremendous contortions of its surface, and belchings forth of the yet molten matter beneath, such

as yields legitimate material for the imagination, and the most sublime subject for reflection. The extraordinary contortions and confusion into which the more primitive sedimentary strata, such as the gneiss, schist, and mica slate, are found to have in so very indistinct degree, shadow forth the state of things which must have existed during that period, when the ocean held a very disputed residence on the surface of the earth.

"Could the surface of the earth have been viewed at that era of its geological history from such a distance as the planet *Mars*, I doubt not it would have yielded an aspect in no very dissimilar respect to that which we now observe in the case of *Jupiter:* namely, that while the actual body of the earth would have been hid by the vast vapour envelope then surrounding it, the tremendous convulsions going on within this veil would have been indicated by streaks and disruptions in the belts; and those, again, mottled over with markings, such as we observe in the case of the entire surface of *Jupiter:* and by reason of the belchings forth of the monstrous volcanoes which at that period must have been so tremendously active on the earth, the vapour envelope would be most probably marked here and there with just such dingy and black-and-white patches, as form so remarkable features about the equatoreal region of *Jupiter*—probably the result of volcanic matter, such as ashes, &c.—which the several volcanoes may from time to time vomit forth, and send so far up into its cloudy atmosphere as to appear on the exteriors, and so cause those remarkable features which so often manifest themselves on the exterior of his vapour envelope; for I doubt if we have ever seen the body of *Jupiter* yet, which will probably remain veiled from mortal eyes for countless ages to come, or until he be so cooled down as to permit of a permanent residuum on his surface of his ocean that is to be.

"In applying these views to *Saturn*, it occurs to me that we may in that respect obtain some glimpse into the nature of those causes which have induced, and are now apparently inducing, those changes in respect to the aspect of his rings, which have, more especially of late, attracted so much attention. If he also be so hot, that his future ocean is also suspended as a vast vapour envelope around him, it is possible, I conceive, that some portion of this vapour may migrate, by reason of the peculiar electrical conditions which it is probable his rings may be in, in respect to the body of the planet; and that such migration of vapour in an intensely frozen state, as it must be in such situation, may not only appear from time to time, as the present *phantom ring* does, but also encrust the inner portion of the inner old ring with such coatings of hoar-frost, like crystallised water, as to cause the remarkable whiteness which so peculiarly distinguishes that portion of his rings. Such, in fact, are the extraordinary phenomena of this planet, that one is led to hazard a conjecture or two on the subject; and, I trust, such as I have now the pleasure to offer may have a kind reception from the Royal Astronomical Society.

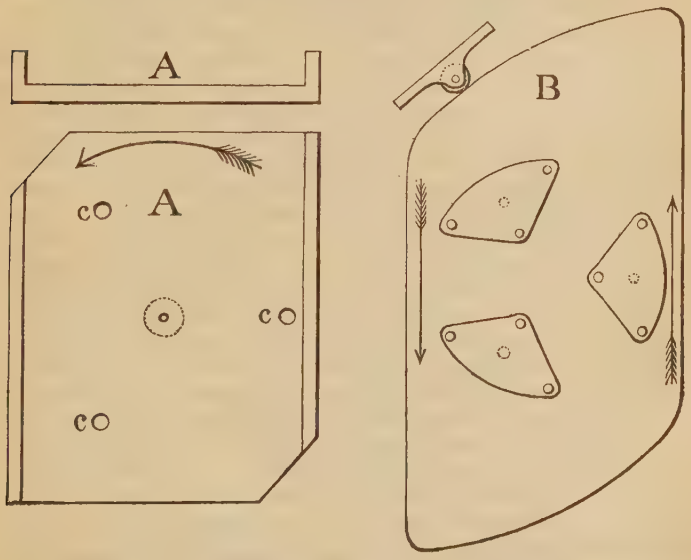
"*January* 13, 1853."

Description of an Improvement in his Machine for Polishing Specula. By Mr. Lassell.

"During a pretty extensive use of the polishing machine described in the eighteenth volume of our *Memoirs*, and while seeking especially a higher excellence of defining power than I had up to that time attained, I noticed that the first progress of incipient polish manifested an *annular* structure, and, therefore, gave unequivocal evidence of some want of uniform action over the whole surface of the speculum.

"In some instances I have noticed the lustre first appearing in a central spot of perhaps a tenth part in diameter of the diameter of the speculum, and surrounded by six or eight annuli of partially polished surface, alternating up to the edge of the speculum with annuli of less reflective surface. As the polishing process advanced, this inequality of lustre became less evident, and when completed no trace of it was apparent. But, of course, the inference was inevitable, that the same irregularity must exist, though not manifest to the eye; and I forthwith became persuaded that it would be necessary, if possible, to introduce some transverse action, in order to wipe out, as it were, these rings, which, it appeared, the purely circular motion of the quick and slow-moving cranks had a tendency to produce.

"This end has been accomplished with, I think, perfect success, by a simple apparatus introduced between the upper surfaces of the vertical spindle and the speculum itself, leaving all the motions of the upper cranks and other parts of the machine untouched.



"In the annexed figures, A A are two views of a cast-iron plate

about half-inch thick, which, by the three holes C, drops upon three studs screwed into the upper surface of the wheel of the vertical spindle carrying the speculum. B is a plate about the same thickness as A, placed on A's surface, and easily sliding within the ribs cast on the edges. On the upper surface of B, the speculum rests on its plates. The ends of B are curved, as in the figure, and as the plates A and B revolve, carried by the vertical spindle, the curved ends of B are *each* brought, *once* in a revolution, in contact with a pulley or roller fixed in the upright wall-plate of the machine, and adjustable as to proximity. The contact of the roller forces B to slide, smoothly and regularly, athwart A, producing the effect of a regulating or correcting cross stroke, amidst all the circular ones, and quite annihilating all ring-like tendency. The amount of lateral thrust necessary is not great, and I have used from one inch with a nine-inch speculum to one and a half or two inches with a two-foot speculum. With a shorter proportion of focus, less than this would be sufficient; for its effect is to reduce in some degree the parabolic tendency of the machine.

"The improvement of definition, as well as of certainty in the operation, which this simple arrangement produces, is most striking, and gives, I would almost venture to say, the last touch of perfection to the machine.

"*Valetta, 24th November*, 1852."

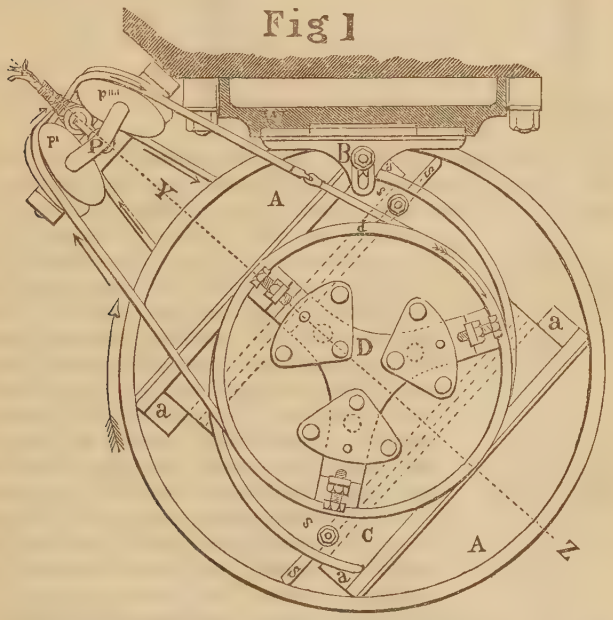
On the Figuring of Specula. By Warren De La Rue, Ph.D., F.R.S.

It has been said of the reflecting telescope, that "no one, competent to form a judgment on the subject, was ever satisfied with such an instrument, excepting the man who made it." This remark, inasmuch as it may be referred to the definition of a fixed star, is so true, that, in my own case, I include the maker of the instrument as also one of the dissatisfied parties. At the same time it must, however, be stated, that I usually succeed in producing thirteen-inch mirrors, which define the planets *Mercury*, *Venus*, *Mars*, *Jupiter*, *Saturn*, and *Uranus*, with the surface of the moon, and particularly its *bright* limb, in a manner rarely equalled, and never surpassed, by any of the refractors which I have yet had an opportunity of looking through. I am, however, free to confess, that they defined a fixed star much more satisfactorily than my best mirrors.* I am, therefore, inclined to believe that it requires a far more accurate figure in order to show a *star free from rays*, in the case of a reflector, than in that of a refractor. Difficult as the attainment of this object has hitherto proved, recent results of my own, as well as those obtained by Mr. Lassell by means of his "transverse-motion," lead me to hope that the obstacles in the way of that attainment are not insurmountable. The design of the present communication is, indeed, to bring under the notice of the Society two additions to Mr. Lassell's polishing-machine; which,

* I have seen the components of ω *Leonis* neatly separated by the mirrors to which I allude.

in conjunction with the "transverse-motion," have enabled me to produce figures showing as discs the components of such stars as *γ Leonis* and *α Geminorum*, whenever the state of the atmosphere admits of the use of magnifying powers from 500 to 700. With a low power (150) a bright star is surrounded by rays, so faint, however, that they appear reddish in comparison with the intense brilliancy of the nucleus.

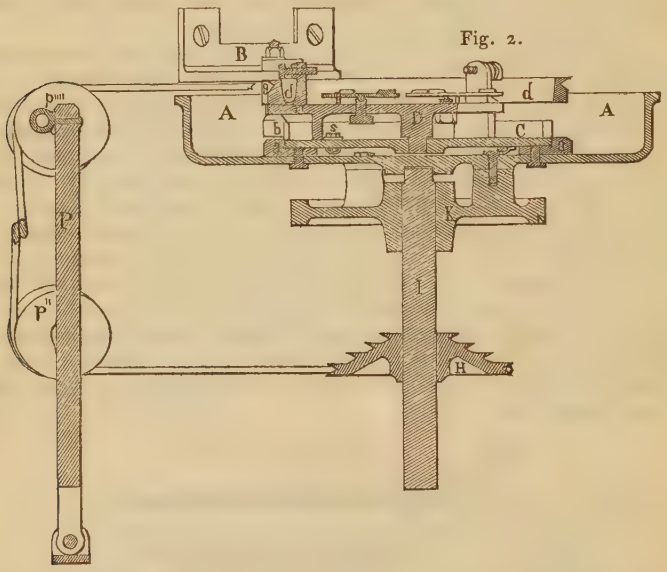
By referring to Mr. Lassell's description of his "transverse-motion," it will be perceived that the cross-stroke occurs always in the direction of the same diameter of the speculum; a result which it appeared to me very desirable to obviate, inasmuch as the recurrence of the change of excentricity at precisely the same epoch must have some tendency, however small, to produce an irregularity of figure. In adopting the "transverse-motion," I therefore superadded a very simple contrivance for causing the speculum to rotate independently of the cam-plate. This arrangement is delineated in figures 1, 2, and 3, which are drawn to a scale of one inch to the foot, and also represent so much of the machine as is necessary to a clear comprehension of its position in relation to the other parts. Figure 1 is a plan; figure 2 a sectional elevation through the dotted line Y Z of figure 1; and figure 3 a back-view of a swinging pulley-frame.



I is the lower spindle of the machine, on which is keyed the worm-wheel K, to three projections from which is screwed the pan A. On the bottom of the pan are screwed two guides, *a*, be-

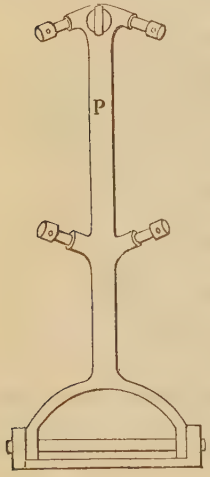
tween which the cam-plate, *c*, slides, when acted upon by the roller *b*, in consequence of its own rotation in the direction of the arrow. The roller *b* turns freely on a spindle, adjustable, for the purpose of altering the throw, in the slotted plate B, which is fastened to the wall-plate W, shown in figure 1, but not in figure 2.

Fig. 2.



The tripods for carrying the speculum, instead of resting upon the cam-plate, as proposed by Mr. Lassell, are supported by the capped ends of three screws passing through the arms of a tripod-stand D, to which arms is attached a light wrought-iron rim *d*, grooved on its periphery so as to receive a gut. An axis projects from the centre of the arms, and enters a hole in the cam-plate: its shoulder does not, however, bear on its surface; so that the arms are supported solely by the three legs projecting from their under-side. In order to prevent the speculum from being carried round by the cam-plate, a crossed endless gut is led round the rigger *d*, and then over the guide-pulleys p' and p'', round the speed-rigger H, keyed on to the spindle I, and then over the guide-pulleys p''' and p'''', back to *d*. The four guide-pulleys are carried by the swinging-frame P; and to an eye at the back of this frame is attached one end of a rope, which passes over a pulley, and carries a tightening weight at its other end. In consequence of this arrangement,

Fig. 3.



the speculum, instead of rotating in the same time as the cam-plate, has, on the contrary, a slower motion, though revolving in the same direction.* The cross-strokes can, therefore, recur along the same diameter of the speculum only after many revolutions, on account of the ratio of the diameters of the two riggers *d* and H; which, for the greatest diameter of the latter (that hitherto used) is as 1·69 : 1; the cam-plate rotating 169 times, while the speculum rotates 100 times.

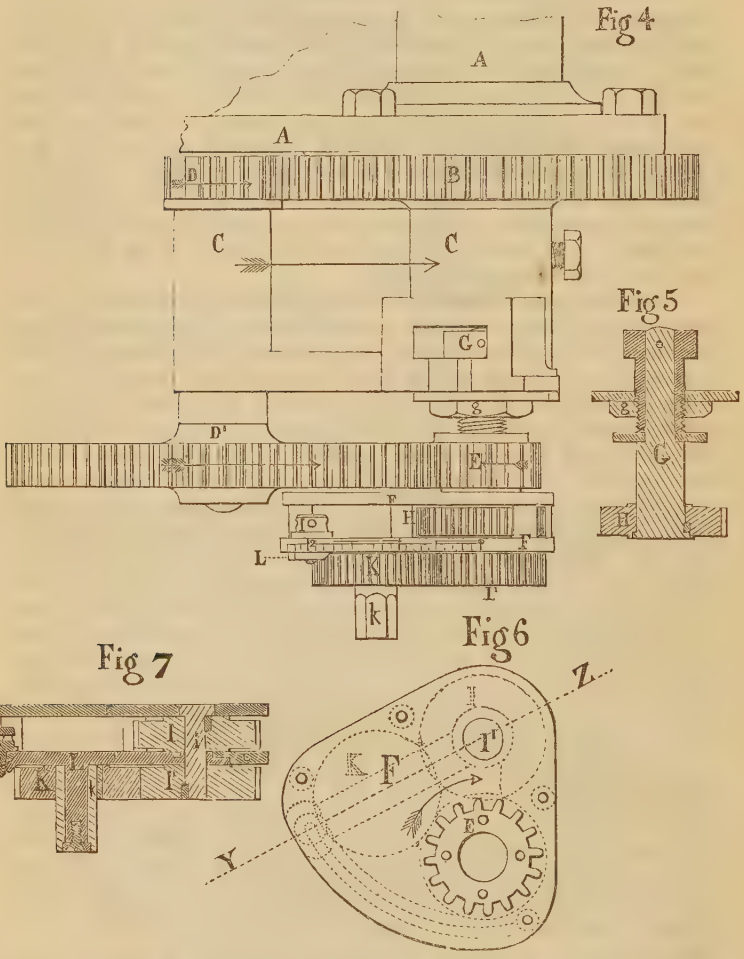
The swinging-frame P is so placed, that its pull is perpendicular to the guides *a*, when the cam-plate has completed its stroke; the tightening-weight, therefore, cannot then move it in any direction. As the pan continues its rotation the weight would, however, pull the cam-plate towards its rim, and increase the throw beyond that given by the roller *b*, unless prevented. To avoid such an occurrence there are two stop-bars, S, adjustable beyond the cam-plate to suit the various throws which may be given to it; namely, from 0 to 2·2 inches on each side of the centre. Each stop passes through a fixed guide at one end and a screwed-staple or eye, *s*, at the other; by drawing this eye tight by means of the nut *s*, the stop is held securely in its place.

None who have used a Lassell's polishing-machine can have failed to be struck with the uncertainty and irregularity of the motion of the polisher around its axis, the quick-crank pin; and to have experienced a strong desire to have this motion as much under control as all the other movements of the machine. The polisher, being quite free to rotate, usually zigzags in one or other direction; but seldom rotates smoothly, and certainly never with any approach to a constant velocity-ratio, however attentive the operator may be in regard to the due distribution of rouge and water, for the purpose of rendering the resistance as uniform as possible. This irregular action did not escape the observation of Mr. Lassell, as he some time since proposed a plan for driving the polisher by means of a gut or band from a pulley working on an axis affixed to a horizontal swinging frame, which, moving to and fro as the polisher was carried round, maintained a constant radial distance between the polisher and its driver. Mr. James Nasmyth, in describing this arrangement to me, in a letter dated October 9th, 1850, proposed another plan. It consisted in fixing a series of long pegs around the periphery of the polisher, and placing a stationary stop in such a position that one peg after another would come in contact with it, each time the polisher was swung towards it, thus causing the polisher to move through a small arc at every throw. After having tried several schemes with indifferent success, I devised the apparatus presently to be described, which is so simple, that I am only surprised it did not occur to me from the first.

Figures 4, 5, 6, and 7, drawn to a scale of three inches to the

* It is my intention to make a slight addition to the swinging-frame, in order to allow of an open gut being used, so as to have the facility of driving the speculum in the contrary direction also.

foot, are different views of a new quick-crank in substitution of that employed by Mr. Lassell. Figure 4 is an elevation of it, and of as much of the other parts of the machine as is required for the comprehension of the position of the new crank in respect to them. Figure 5 is a section of the crank-pin, and a fixed wheel, which is keyed thereon. Figure 6 is a plan of the new crank removed from the pin shown at figure 5; and figure 7 a sectional elevation of it, through the dotted line Y Z of figure 6.



A is the upper fixed bracket screwed to the wall-plate, and carrying the upper spindle of the machine. On this spindle is fixed the train-bearing arm C C, which revolves in the direction of the arrow. B is a wheel of 75 teeth, screwed fast to the fixed

bracket A, and geering with the pinion D of 16 teeth. The pinion D, and the wheel D′ of 75 teeth, are keyed on to a common spindle, which rotates in the arm C. The wheel D′ geers into the pinion of 16 teeth E, to which is fastened the triangular crank-frame F, composed of two wrought-iron plates, kept apart by four pillars, which are rivetted into them. The pinion E, and, consequently, the frame F, rotates freely on the spindle G, with a velocity ratio in respect to that of the arm C, represented by $\frac{75 \times 75}{16 \times 16} : 1$. Hence it would produce 21·97265 hypocyclic loops for one revolution of the upper spindle; on the supposition, however, that the lower spindle I, fig. 2, remains at rest: but as motion is given to this usually in a contrary direction by means of a gut working in one or other of the grooves of a pair of speed-pulleys (one on the upper and one on the lower worm-shaft), the number of loops varies with the velocity with, and the direction in, which it is driven.

The crank-pin G, which it will be remembered, from Mr. Lassell's description of his machine, may be adjusted so as to be either concentric with the upper spindle or at any degree of excentricity within the range of the machine, namely, five inches, is seen best in the sectional view, fig. 5. It is composed of the spindle proper and a hollow socket, into which this is held by a removable pin; the lower part of the socket being screwed, and carrying the nut *g*, surmounted by a washer: between the shoulder of the spindle proper and the end of the screwed socket is inserted a washer, so as to form a good bearing surface for the pinion E. On the lower end of the spindle G is keyed the wheel H, of 52 teeth. I and I′ are two wheels, each of 52 teeth, keyed on to a spindle *i*, which rotates freely in the rectangular crank-frame F; the wheel I geering in the fixed wheel H, receives a rotatory motion as the crank-frame carries it round it; I′, fixed on the same spindle as I, geers into the wheel K, having also 52 teeth: this is fastened on the hollow steel spindle *k*, which rotates freely on the spindle L, projecting from, and being a part of, the radial arm L. L is an arm moving on the spindle *i*, like a rule-joint, and adjustable by means of the circular slot at a position either concentric with the crank-pin G, or at any excentricity up to two and a half inches, the lower plate of the crank-frame being pierced with that view to an extent indicated by the dotted exterior outline of the arm L, the inner circular arc, the exterior outline of the wheel H, and the radial line joining it (see fig. 6). The screw *l* serves to hold the arm L firmly in its place. The effect of the epicyclic train H I, I′ K, the teeth of each wheel being equal in numbers, is to annihilate so much of the rotary motion of the spindle *k* as arises from its revolution around G, and hence to produce parallelism in respect to the arm C C; it, however, continues to rotate relatively to a fixed point of the machine once for every revolution of the main arm C C. The lower part of the spindle *k* is made hexagonal, in order to prevent the polisher from rotating without control; it is, however, quite at liberty to move up and down, so as to follow the

curvature of the speculum. As the rotation of the spindle *k* is compensated in regard to the arm C C, it follows that any line of the polisher parallel with it always retains this parallelism, and, consequently, that every point on its surface describes the same number of hypocyclic loops, in round numbers 22; but while the loops described by the centre are generally nearly circular, those near the periphery are much elongated.

If the polisher were made to maintain its parallelism with a fixed line in a space, and the speculum remained at rest, then every point of the polisher's surface would describe precisely the same figure. Such parallelism might be obtained sufficiently near, for practical purposes, by making the number of I″'s teeth any multiple of 21, and those of K any multiple of 22; for instance, by giving to I′ 63 teeth, and to K 66, because one rotation in twenty-two of the crank-spindle *k* is compensated by the revolution in the opposite direction of the arm C C, which carries the whole system.

These numbers would not, however, suit in the case of the speculum revolving, which it always does, but must be calculated for each velocity of the speculum; taking also into account the direction of its motion in relation to the arm C C.

I have not at present a train to produce an *approximate* parallelism (*absolute* parallelism would not be desirable) in relation to a diameter of the speculum; but I have two, besides that of equal wheels already described, either of which produces such an amount of rotation in the polisher as I have observed it take up when left uncontrolled, namely, No. 2, in which H has 52 teeth, I 52, I′ 54, and K 50: it is, therefore, represented by the fraction $\frac{52 \times 54}{52 \times 50} = 1{\cdot}08$. When governed by this train the polisher rotates in relation to the fixed frame of the machine 2·76 times for one revolution of the arm, and in the same direction; hence, in relation to the arm, it rotates in the ratio of 1·76 : 1, and describes an epicycloid of two loops, or, more exactly, fifty-eight loops for thirty-three revolutions of the arm. Twenty-five hypocyclic loops, due to the excentricity of the spindle *k*, are distributed around this curve by the periphery of the polisher, whereas its centre distributes them around a circle, and produces a hypocycloid of twenty-two loops, precisely as is done by train No. 1; but it moves through 1·14 revolution while the periphery rotates once.

No. 3 train, in which K has 52 teeth, I 52, I′ 51, and K 53; it is, therefore, represented by the fraction $\frac{52 \times 51}{52 \times 53} = \frac{51}{53} = 0{\cdot}962264$. When governed by this train, the polisher in relation to the fixed frame, rotates in the same direction as the arm with a velocity ratio of 0·172 for one revolution of the latter. The arm therefore revolves, in relation to it, with a velocity of 828 : 172. The result of these relative speeds is the production, by a point in the periphery of the polisher, of an epicycloid of five loops, or, more exactly, thirty-four loops for seven rotations of the polisher; for in this case the excentricity of the pin G becomes the

radius of the epicycle. Around this epicycloid of five loops the hypocyclic loops due to the excentricity of the spindle *k* are so distributed by a point in the periphery of the polisher that for one complete rotation of it 129 loops are produced, while its centre describes the hypocycloid of twenty-two loops, as in the former case; but these are repeated 5·814 times for one rotation of the periphery of the polisher. This is the only train I have yet used in polishing, and is that which has given the good results alluded to at the commencement of this paper.

The various figures described in the preceding paragraphs are very much modified by the cross-motion of the machine, and also by the rotation of the speculum; but it would occupy too much space to describe the multiplicity of changes which arise.

From the foregoing statements it will be seen, that small differences in the rotation-velocity of the polisher must have a material effect in altering the relative grinding action of points at various radial distances on its surface; and hence, to ensure the reproduction of a desired result, this velocity must be taken into account as well as any other adjustable ratio in the machine.

With the view of studying the effect of any given rotation-velocity of the polisher in relation to all or any of the movements of the machine, I fix an arm on the hexagonal pin *k*, so as to represent a radius of the polisher. On this arm slide three pen-holders, in either or all of which I can place cylindrical pens, which are charged with inks of different colours. On putting the machine or any part of it into motion, the pens trace out the curves due to all or any of the movements, and thus afford a ready means of ascertaining the distribution of grinding effect. As the result of several trials of this kind, I may state that so great a difference is produced in the various figures by comparatively small alterations in the adjustments, that I believe this mode of investigation will prove of the highest value to the telescope-maker, by enabling him to form a good conjecture beforehand of the results likely to arise from various settings of the machine.

I desire to avail myself of this opportunity to acknowledge my obligations to Mr. Lassell for his ready communication, from time to time, of his methods of manipulation. I have also to express my acknowledgments to Mr. James Nasmyth, for many most valuable hints in polishing and grinding, more particularly for imparting to me his plan of producing a beautifully uniform surface to specula, previous to the final pitch-polishing, by means of a grinder made up of squares of German blue-stone (hone), cemented with a composition of pitch and rosin on to a light frame of cast-iron. I attribute much of any success which I may have had to the adoption of this plan, or a slight modification of it. My alteration consists in substituting a slab of common slate for the bed of hones; the former having the advantage of always retaining its figure, which the hone-tool does not when laid on one side for any length of time. On a future occasion I may have something further to communicate respecting the employment of the slate-tool.

New Solution of Kepler's Problem. By Professor P. A. Hansen.

(*Extract from a Letter to the Astronomer Royal.*)

"It is well known how much labour has been bestowed by geometers on the solution of Kepler's problem; and what complicated results have been obtained for the coefficients in the expression for the equation of the centre. I have lately found a new solution of this problem, which differs strikingly from former solutions in this respect, that it leads to an unexpectedly simple law of coefficients. It is as follows:—

Let g be the mean anomaly.
φ the angle of eccentricity.
i any positive integer number.

Put $\beta = \tan \frac{\varphi}{2}$, $\quad \mu = i \cos^2 \frac{\varphi}{2}$

$$P_i = 1 + \mu + \frac{\mu^2}{2} + \frac{\mu^2}{2.3} + \text{&c.} + \frac{\mu^i}{2.3 \ldots i} \qquad Q_1 = 1 - \mu$$

$$P_{i+1} = P_i + \frac{\mu^{i+1}}{2.3 \ldots i+1} \qquad Q_2 = Q_1 + \frac{\mu^2}{2}$$

$$\text{&c.} \qquad Q_3 = Q_2 - \frac{\mu^3}{2.3}$$

$$\text{&c.}$$

Then the equation of the centre =

$$(1-\beta^2)\, \Sigma_1^{\infty} \frac{2}{i} \left\{ P_i \,.\, \beta^i + P_{i+1} \,.\, Q_1 \,.\, \beta^{i+2} + P_{i+2} \,.\, Q_2 \,.\, \beta^{i+4} \,. \right.$$

$$\left. + \text{&c.} \right\} \sin i g.$$

"The analysis which has led me to this, and the form of this result, are by no means peculiar to the equation of the centre only, but apply to all functions which it is necessary to develope in series proceeding by sin $i g$, or cos $i g$."

On the Brilliancy of the variable Star α Orionis. By Mr. Isaac Fletcher, Tarn Bank.

"In a communication read before the Royal Astronomical Society on January 10, 1840, Sir John Herschel points out and establishes by indubitable evidence the variability and periodic nature of the star α *Orionis*. In that paper, Sir John states as the result of his numerous observations and comparisons, that the brilliancy of this star, when at its maximum, is superior to that of

Rigel, but most decidedly inferior to that of *Capella*, which of late years has undoubtedly been the brightest star in the northern hemisphere.

"On the evening of the 5th December, 1852, I was much struck with the extraordinary brightness of *α Orionis;* and notwithstanding its inferiority of altitude, on carefully comparing it with *Capella*, I had no hesitation in concluding *α Orionis* to be the brighter star. Nor was this to be attributed to any diminution of lustre in *Capella;* for in the earlier part of the evening, when *α Lyræ* and *Capella* were of nearly equal altitude, I compared them together, and *Capella* was unquestionably the more brilliant.

"As regards *α Orionis* and *Capella*, on the night in question, I appealed to the judgment of two friends, both of whom deliberately and repeatedly compared the stars together, and assigned the superiority to *α Orionis*. On the 6th December I made another comparison; and although I thought the difference in brightness was less obvious than on the previous evening, still I did not hesitate to assign the superiority to *α Orionis;* such was also the judgment of another person to whom I referred the matter.

"Next evening I again compared the stars, and although the neighbourhood of *Orion* was occasionally obscured by fog, I concluded *α Orionis* to be fully equal, if not superior, to *Capella*. The following evening the sky was too hazy for accurate comparison.

"I, however, consider this fact to be clearly established; viz. that on or about the 5th of December, 1852, *α Orionis* was at or near its maximum brilliancy, and that at that time *it was the brightest star in the northern hemisphere.*

"I hope the circumstance I have above recorded will induce those observers who are watching the phenomena of the variable stars, to pay particular attention to this object.

"*December 9th*, 1852."

Mr. W. Rothwell exhibited a small transit instrument on a peculiar construction.

The telescope, which is only 14 inches long, is fixed on a brass pillar, so as to look downwards at about an angle of 45°; it thus somewhat resembles in appearance a solar microscope as usually mounted. The brass pillar is attached to a horizontal plate, which has the necessary adjustments for setting it level, and also for giving it a small motion in azimuth. In the line of sight of the telescope, and pretty close to the object-glass, there is a horizontal axis at right angles to the telescope. This axis rests on Ys as an ordinary transit; it has the same adjustments and riding level. In the centre of the axis is placed a mirror, like the index-glass of a sextant, into which the telescope looks for all its objects. The instrument being placed approximately in the meridian, and the

horizontal plate and the axis being carefully levelled, the axis is turned round until the wires of the telescope, which are illuminated by a side-lamp, are seen by reflexion from the mirror. The wires seen thus reflected are made to coincide with the wires seen *directly*, by the screws at the eye-end, in the usual manner. The axis is then reversed, and the reflected and direct wires again made to coincide,—half by moving the collimation adjusting-screws, and half by the two screws at the back of the mirror. When the axis is returned to its original position, if the direct and reflected wires coincide, the reflecting surface will be perpendicular to the optical axis and parallel with the horizontal axis.

At one end of the horizontal axis there is an altitude-circle, which is fitted exactly as in the usual portable transit; the vernier shows the angle through which the mirror is turned. The mirror must be turned round till a known star is found and bisected by the horizontal wire, when a comparison of the vernier reading with the known polar distance of the star will enable the observer to set the vernier to the exact reading, north polar distance, or altitude, or zenith distance, which he requires.

The whole instrument must be set in the meridian by making the stars near the south horizon give the same clock-error as the stars near the zenith. Mr. Rothwell recommends this form of transit, not account of its superior accuracy, but superior convenience, and considers it as sufficient for ordinary purposes. The stand would probably be less liable to change if the mounting were *cut out of stone*. With nice workmanship, an excellent mirror, and great care, considerable accuracy might no doubt be attained. A meridian-mark would be a desirable check for rating time-keepers, if any well-defined object could be found or placed in the meridian.

Mr. Norman Pogson, who is now attached to the Radcliffe Observatory, writes, that after his regular work for the evening is over (transit-observing), he employs the equatoreal, on fine nights, in searching for new planets, using Mr. Bishop's and the Berlin charts.

Mr. Pogson uses two diaphragms. No. 1 has a vertical bar, which passes through the field in the direction of a meridian, straight on one side, and toothed on the other. The transits are observed at the straight edge, and the differences of polar distances noted approximately at the toothed side: the teeth are about 2′ apart. No. 2 is adapted for observing faint objects. This consists of two bars arranged as a V; one of these is set in the meridian, and on this the transits are taken; the time between the two bars gives, by a slight computation, the distance of each star from the angular point. The angle may be varied to suit specific objects, as was done by Lacaille, but for ordinary purposes 45° is a convenient opening.

The telescope is also furnished with a good ring micrometer,

with which Mr. Pogson made the observations of the small planets *Fortuna* and *Melpomene*, *Monthly Notices*, vol. xii. pp. 214, 217. The reductions are rather tedious, but when the difference of declination is less than 4′ or more than 6′, Mr. Pogson considers the results may be relied on. The different measures of the diameter of the ring agree very well.

Capt. Shea has presented some suggestions *on the cause of the large quantity of rain* which has recently fallen. His reasoning does not assign the phenomenon to celestial agency, but refers to the condition of the north polar region of the earth; the notice belongs, therefore, to meteorology rather than to astronomy.

In vol. 36 of the *Astronomische Nachrichten*, No. 844, p. 67, M. Luther, of the Bilk Observatory, has given an approximate ephemeris of *Parthenope* for the present opposition. The ephemeris is for Berlin mean midnight, and extends from Jan. 16 to Feb. 25, 1853.

Astronomical Observations made at the Radcliffe Observatory, Oxford, in the year 1850. By Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XXI. royal 8vo. (pp. 300, Introduction pp. xliv). Oxford, 1852.

The explanations necessary for a due appreciation of this work are given in the Introduction. The observations were made chiefly in the meridian, but there are a few determinations with the heliometer.

A detailed description of the meridian instruments has been given in former volumes. The great mass of the objects observed are circumpolar stars. Subordinate to these are the observed places of a limited number of fundamental stars, for the determination of instrumental and clock errors. There are also a few observations of the sun, as well as of the moon and moon-culminating stars.

The results of the circumpolar observations are presented in a catalogue adapted to the mean equinox of 1850·0. The number of stars is 2009. This contribution will serve nearly to complete the important catalogue of circumpolar stars which for several years has occupied so much of Mr. Johnson's attention. The re-observation of Groombridge's Catalogue may be now considered to be finished, and Mr. Johnson has appended to this volume an "Index of Stars in Groombridge's Catalogue, observations of which will be found in the eleventh and preceding volumes of the Radcliffe Observations." Until the Radcliffe Catalogue shall be published in a collected shape, which may soon be expected, this index will enable any one to determine the place of a star with very little trouble.

The circumpolar catalogue is followed by a smaller catalogue

of fundamental and other stars, whose north polar distances exceed 50°. The number of these stars is eighty-five. After this catalogue there is inserted a comparison of the observed right ascensions and north polar distances of the sun and moon, with the corresponding results of theory, as deducible by interpolation from the *Nautical Almanac*.

The concluding part of the work is devoted to the observations with the heliometer. There is a detailed description of this instrument in the Introduction, accompanied with an engraving at the beginning of the volume. The observations, having been made for studying the peculiarities of the instrument rather than with reference to any particular astronomical purpose, are not published from any conviction of their intrinsic value. They consist of a few measures of double stars, and of the apparent diameters of *Mars* and *Jupiter*. The methods of measurement varied with the objects observed. For double stars, whose components were situate at a moderate distance, the objects were ranged in lines at equal intervals, the moving image being placed alternately east and west of the fixed image. If the distance was considerable, the components of the moving image were brought alternately midway on the line of distance between the components of the fixed image. Generally, however, in such cases the observations were made by contact. The number of double stars observed is twenty-six. There are, however, several measures of each star. The mean result of eight measures of *γ Virginis* assigns 2″·851 as the distance of the two components for the mean epoch, 1850·230. There are a few measures of *Mars*, which were executed in the beginning of January. They give 5″·901 and 6″·503 for the mean values of the polar and equatoreal diameters. From the measures of *Jupiter* Mr. Johnson has deduced an ellipticity amounting to $\frac{1}{15\cdot6}$.

Researches on the Apparent Diameter, the Ellipticity, and the Position of the Axis of Rotation, of the Planet Mars. By Dr. J. A. C. Oudemans, of Leyden. (*Ast. Nach.* No. 838.)

The observations which form the groundwork of these researches were made by Bessel at Königsberg, with the great heliometer. The interval embraced by them extends from 1830 to 1837. As they correspond to very different distances of *Mars* from the earth, they were deemed by M. Oudemans to constitute favourable materials for investigating the circumstances relating to the magnitude, figure, and rotation of the planet. No sensible indications of ellipticity were afforded by the observations. The mean value which they assigned to the semi-diameter corresponding to a distance equal to the mean distance of the earth from the sun, was 4″·664. The inclination of the axis of rotation to the ecliptic was found to be 61° 9′, and the longitude of the ascending node of the planet's equator to be 349° 1′.

Briefwechsel zwischen W. Olbers und F. W. Bessel, herausgegeben von Adolph Erman, in zwei Bänden. 8vo. Leipzig, Avenarius and Mendelsson, 1852.

The first volume contains a short preface by the Editor, v.–viii.; an autobiography by Bessel, up to the age of twenty-five, ix.–xxxvi.; and a catalogue of the letters with their dates, xxxvii.–xxxix. The correspondence, which commences with a letter from Olbers to Bessel, dated August 12, 1804, fills 448 pages of the first volume.

Volume II. has a catalogue of the letters with their dates, v.–vi., and then the correspondence in 442 pages, ending with a letter from Bessel to Olbers, 28th July, 1839.

The work altogether contains 279 letters by Olbers, and 318 by Bessel, almost all relating to astronomical subjects. To those who are at all acquainted with the writers, it is needless to say that this work *must be read* by every one who wishes to acquaint himself with the progress of astronomy in the present century.

The Achromatic Telescope and its various Mountings, especially the Equatoreal, &c. By William Simms, F.R.S. F.R.A.S. &c. 8vo. 74 pp. London, 1852. Troughton and Simms, 138 Fleet Street.

In this brief work Mr. Simms has explained the principles on which the achromatic telescope depends, and has described and figured the parts of which it is composed. He has given drawings of most of the equatorial mountings now in use, with their appendages, and sketches of a suitable observatory. The volume is adapted to the use of amateur astronomers, and in itself professes nothing more; but there are references to other works of a more extended scientific character.

Recherches sur le prochain Retour de la Comète de D'Arrest. Par M. Villarceau.

(*Comptes Rendus, Dec.* 6, 1852.)

Two sets of elements are assigned, the results in each case being affected by an indeterminate quantity. Supposing this quantity equal to zero, the next passage of the perihelion is fixed for 1857, Dec. 2·368, or 1857, Nov. 28·742, according as the one or the other set of elements is adopted.

Notice sur quelques Recherches récentes Astronomiques et Physiques relative aux Apparences que présente le Corps du Soleil. Rédigée par M. le Prof. Alfred Gautier. Geneva, 1852.

In this *brochure* M. Gautier has collected a variety of interesting facts relative to the solar spots and other kindred subjects, tending to throw light upon the physical constitution of the sun.

Troisième Mémoire sur les Etoiles Doubles. Par M. Villarceau.

According to this method the five coefficients of the apparent orbit are deduced from the *apparent* co-ordinates. The mean motion and the longitude of the epoch are then obtained by means of the time and the law of the areas.

Théorie Analytique de l'Inégalité de Lumière des Etoiles Doubles. Par M. Villarceau.

In his memoir upon double stars (*Connaissance des Temps*, 1830), Savary first drew attention to the inequality depending on the difference of the times which light occupies in passing from the two components of a double star to the earth, and he suggested the possibility of hence deducing an inferior limit of the parallax of the star. M. Villarceau having undertaken an analytical investigation of the subject, obtained four inequalities depending upon this principle. The first of these depends simply on the distance of the star from the sun; the second, on the circumstance of the star's approach to, or recess from, the sun; the third, on the proper motion of the star; and the fourth, on the relative masses of the two components. The three inequalities first mentioned are confounded with the elliptic motion of the star. The fourth, which depends on the relation between the masses of the stars, had not been hitherto recognised by any one. If this relation was known, the parallax which is involved with it in the equations of condition might hence be deduced. Since, however, this element is totally unknown, it is impossible to establish even an inferior limit of the parallax by means of such researches.

Note de M. Faye, sur une nouvelle Détermination de la Parallaxe de l'Etoile d'Argelander (1830, *Groombridge*). Par M. Wichmann.

(*Comptes Rendus, Dec.* 13, 1852.)

It is well known that M. Faye, by comparing the right ascension of this star with that of another small star situate nearly on the same parallel (whose parallax was supposed to be insensible), found its parallax to amount to $1''{\cdot}08$. Subsequently, however, M. Peters, by observations of declination, obtained a parallax of only $0''{\cdot}28$. M. Otto Struve, by comparing the observed declinations of the star at different seasons of the year with the corresponding declinations of two other small stars in its vicinity, found the parallax to be $0''{\cdot}03$; and M. Wichmann, from observations made at Königsberg with the great heliometer, deduced a parallax amounting only to $0''{\cdot}18$.

The observations with the Königsberg heliometer were made by comparing the star of Argelander (A) with two other small stars, a, a', whose parallaxes were supposed to be insensible, and the

parallax of the star A was determined by a discussion of the *differences* of its distances from the two stars of comparison. The *sum* of the distances, however, which ought to have been constant, exhibited an annual variation which could not be accounted for by any recognised principle. The hypothesis of this variation being attributable to the influence of temperature upon the different parts of the instrument leads to improbable conclusions. If, however, *one of the stars of comparison* be supposed to have a sensible parallax, the periodic variation disappears, and the observations are represented with considerable accuracy. M. Wichmann has recently executed a new series of observations with the heliometer, and by a discussion of them founded upon this hypothesis he has found the parallax of A to be 0″·68, and the parallax of *a* to be 1″·15. The former observations, treated in a similar manner, assign 0″·75 and 1″·19 as the parallaxes of the two stars. M. Wichmann's memoir will be found in Nos. 841–844 of the *Astronomische Nachrichten.*

Note on the Mean Density of the Superficial Crust of the Earth. By M. Plana. (*Ast. Nach.* No. 828.)

The researches of geometers have established, beyond doubt, that the density of the earth increases towards the centre. Assuming the densities of the successive strata to increase in arithmetical progression, Laplace has investigated the constant amount of increase for each successive stratum, and has hence deduced the mean density of the terrestrial spheroid (*Méc. Cél.* tome v. liv. xi.). In his researches on this subject, he supposes the density of the superficial stratum (ϱ) to be three times the density of the sea considered equal to unity. He remarks, that this assumption agrees very nearly with the density of granite. His expression for the density of any stratum is,

$$\varrho = (\varrho)\,(1 + e - e\,a),$$

in which a denotes the radius of the stratum (the mean radius of the superficial stratum being supposed equal to unity), and e the constant quantity by which the depth of each successive stratum, $1 - a$ is to be multiplied conformably to the assumed law of density. Admitting the ellipticity of the earth to be equal to 0·00326, Laplace found the value of e to be 2·349, and hence determined the mean density to be 4·764. This value differs considerably from the results which Reich and Baily have deduced from their experiments with the balance of torsion, the former having obtained 5·44, and the latter 5·6604, for the mean density of the terrestrial spheroid, the density of pure water being supposed equal to unity.

The remarks of Humboldt on the density of the superficial stratum of the earth, contained in the first volume of his *Kosmos*, would seem to imply that the value of this element assumed by Laplace is erroneous. He states, that from the nature of the rocks which constitute the superficial strata of the solid parts of the globe, the

density of continents is hardly 2·7, and he hence infers, that the mean density of continents and seas taken together does not amount to 1·6. The researches of Plana, contained in the note above referred to, serve to confirm this conclusion. Supposing the ellipticity of the earth to be represented by 0·00326 (1 — 0·008479), he has found that the mean density 5·44, and the initial density 1·6, may be satisfactorily accounted for. The ellipticity derived either from actual measurement or from researches on the lunar theory cannot be regarded as sufficiently trustworthy to render the value here assumed inadmissible. On the other hand, if the ellipticity be supposed equal to 0·00326, the mean density deducible is 4·76; a result which is incompatible with the precision of the experiments made for the purpose of determining this element.

Metis. — Ephemeris of this planet for the opposition of 1853, extending from September 17 to October 31, will be found in the *Ast. Nach.* No. 845.

Lutetia. — For elements and ephemeris, from January 8 to February 25, by M. Bruhns, see *Ast. Nach.* No. 846.

ERRATA.

Vol. XI., Index, page 250, 12 lines from the bottom, read as follows :—

Dawes, Rev. W. R., discovery of the inner dark ring of *Saturn*, on Nov. 29, 1850..... p. 22 *et seq.*

————————, correction of errors in printing the foregoing accounts... 49

Lassell, Mr., observation of the inner or dark ring of *Saturn*, Dec. 3, 1850... 21

Page 254, line 16,—

Saturn, inner dark ring of, discovery of, by Mr. Dawes, on Nov. 29, 1850.. 22, 49

——, ————————, ————————, by Mr. Lassell, Dec. 3, 1850 21

London:—Printed by G. Barclay, Castle St. Leicester Sq.